FEDERAL SECURITY AGENCY
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

Communicable Disease Center
Enteric Bacteriology Laboratories
P. O. Box 185
Chamblee, Georgia

November 25, 1952

Dr. Joshua Lederberg
Department of Genetics
The University of Wisconsin
College of Agriculture
Madison 6, Wisconsin

Dear Dr. Lederberg:

Thank you very much for your two letters of November 21. I am in agreement with the first paragraph of your first letter.

The second paragraph of that letter raises questions which I think must be discussed at length. Probably we can do this later.

Unfortunately, I am unable to supply you with the culture from which 157 was derived. At least, I believe it will not be possible to do so. We do have some of the S. paratyphi B cultures which were brought here from Kentucky. If it is among these I will forward it to you. If it is not, I can provide a culture of S. paratyphi B var. java from the same outbreak of disease. I imagine that either would be useful to you. We cannot send any z_{33} serum. We have made none of this serum since we were working on induced phases a number of years ago. There is only a very small amount in the laboratory and it is not as high in titre as could be desired. If you wish some serum of this sort I believe you will have no difficulty in producing it from some of the cultures which you have exposed to serum. The culture of the S. paratyphi B No. 6 will be sent.

Thank you very much for the explanation in regard to SW-666. The unusual biochemical reactions of the S. typhi murium cultures are explained satisfactorily in your second letter. Under the circumstances I believe it will not be necessary for you to send your culture SW-435. I agree with your remarks concerning the advisability of using Simmons glucose medium. I presume that you are aware of the extensive work of Hohn and Herrmann on ammonia-weak variants of S. typhi murium.

On rereading your previous letters, I found that it was very clearly stated therein that the cultures which originated from S. typhi came from the 901 culture. I am sorry to have bothered you about this.

The work with your last shipment is going forward and it seems that your results will be confirmed. We prefer to reverse the phases of the diphasic cultures after each phase has been isolated from single colonies. This will take a few days and therefore the report will be

somewhat delayed. Probably I should have explained that your g,p-1,2 form was taken back and forth between the two phases several times. Each time the phase was reversed it was reisolated from a single colony. It may be significant in regard to that culture that it was never possible for us to obtain a form which would not react in g,p serum. On the contrary, it was very easy to obtain a form which would not react in 1,2 serum. This may have a bearing on the remarks contained in the second paragraph of your first letter of November 21.

I quite agree that you have done enough with the H antigen to prove that almost any combination may be possible. I should like very much to see you turn your attention to transformation of O antigens. I think this is a much more difficult problem than transformation of H antigens but I believe the effort would be quite worthwhile.

For the Officer-in-Charge, Bacteriology Section

Sincerely yours,

P.R. Edwards

Philip R. Edwards, Ph. D.
Bacteriologist-in-Charge
Enteric Bacteriology Unit

P.S. We have not tried migration experiments with 157 using single factor 2 serum. For a long time I have been looking for a form which contained two "non-specific" phases. I believe it would be possible for you to produce such a form.

Go ahead and make a 1,2 - 1,5 form. If you produce it we will work out the serology.

Cherry has N25 from which 157 came. We are sending it to you.